

December 12, 1947

Dr. Van R. Potter
McArdle Memorial Lab.
University of Wisconsin
Madison, Wisconsin

Dear Van:

I have received your letter, which is apparently undated, so we will call it the most recent letter.

First, with respect to the theoretical question you raise. I think we have good evidence that the plasmagene and the enzyme are not identical since it is possible to arrange conditions where the rate of enzyme formation is not proportional to the amount of enzymes but to some other component, presumably the plasmagene. Actually, if you will examine the section on the theory of gene action in the Cold Spring Harbor article, a copy of which I am sending under separate cover, you will find that in essence, under normal conditions, what was ~~was~~ concerned with is the plasmagene-enzyme complex so that the rate of enzyme production would be proportional to the product of the two components of the enzyme-forming complex.

Let me hasten to agree that I certainly could have organized my charts a little better, perhaps even a lot better, in the articles you mentioned, but we had some difficulty with the editors in putting in the kind of charts we really wanted, and so I finally got disgusted and let it go the way it is.

Now as to your last question, we have not published data on the rate of adaptation with a wide range of substrate concentrations for a very simple reason. The analysis of the kinetics is complicated by the fact that under usual conditions, the adapting substrate is used as a source of energy for the formation of the enzyme being studied. It was therefore necessary to wait until a procedure or condition could be devised under which the adapting substrate was not supplying the source of energy. Such a condition has been obtained as you remember by the finding that it is possible to get adaptation under circumstances in which the substrate was not being used. This was further extended so that we were able to employ unutilizable analogs of substrate to induce specific enzyme systems. Once that was obtained, it was possible to perform the kind of experiments that you desire and it was performed by Mr. Susman, who finds that he got a typical activity saturation curve which attained its maximal rate

of enzyme induction at about 1.2%. These results, along with the use of the analogs, will be published as soon as I can find the time to write up the final manuscript.

Finally, I am sure you will be interested to know that we have recently made some rather dramatic advances along ~~mice~~ genetic lines. We have been able to obtain a system with which we can with ease duplicate the essential features of the melibiose inheritance story and this system is much easier to analyze and to handle, and we have been able to employ it with great fruitfulness in pushing the experimental test of the plasmagene theory of gene action which I proposed at the 1946 Cold Spring Harbor Symposium. The number of predictions being confirmed, I must confess, is embarrassingly large even to me.

Sincerely yours,

S. Spiegelman

ss/c